Reviewer's report

Title: Adiposity markers and risk of coronary heart disease in patients with type 2 diabetes mellitus

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Reviewer: Nuno n.m.P Pimenta

Reviewer's report:

Comments to the authors:
The authors of this paper did an outstanding job, fully reviewing the paper. At this point this manuscript may be consider for publication. Nevertheless I would still recommend some review points:

Minor essential revisions:
1- In lines 107 and 108, the formulas for conicity index and body adiposity index, respectively, are unreadable. Please format these properly. This may be provided a an image/Figure.
2- In line 208, instead of placing the reference number at the end of the sentence, the reference [13] should appear right after you’ve mention the authors’ name in the text, as follows: “…performed by Ashwell et al. [13], the WHtR is described as a better screening tool than WC to discriminate…”.

Discretionary revisions:
1- In the abstract conclusions (lines 19 and 20) I think the authors mean “The C-index was the body adiposity marker best associated with high risk…”, instead of “The C-index was the best body adiposity marker associated with high risk…”. It’s the association that turn out to be the best and not the body adiposity marker. If the authors do want to focus on the body adiposity markers, we should read something like “The C-index was the best body adiposity marker to predict high risk…”.
2- In line 66 it would be useful if the authors state how was hyperglycemia diagnosed and the respective criteria: fasting blood glucose above 126mg/dl? or glicemia after oral glucose tolerance test above 200mg/dl? Or both? Or other?
3- The paragraph just before the last of the discussion section (lines 226 to 229) should be removed, as it is irrelevant for the results and discussion of the present paper.
4- One thing that would have been interesting to discuss is the fact that, in the present paper, low-moderate risk group have higher prevalence of obesity as compared to high risk group. This underlies the growing evidence regarding the concept of “Obesity Paradox” that have been well described in several specific populations, particularly that with diagnosed cardiovascular diseases.
5- Another question that would be interesting to answer in this paper is as
follows: By excluding all subjects with history of ketoacidosis or documented ketonuria, and no insulin treatment in the first 5 years after diagnosis of diabetes, and including only subjects with serum creatinine <2.0 mg/dl; normal liver and thyroid function tests; absence of urinary tract infection or other renal disease; and absence of severe autonomic neuropathy (presence of symptomatic postural hypotension, gastroparesis or diabetic diarrhea), do the authors think that these results may, to some extent have been influenced by the choice of a more “healthy” sample of type II diabetics, as has been often argued for the Look AHEAD study.

6- One last question that would be interesting to see answered in this paper is why the authors did not use smoking status as a confounder or why didn’t the authors exclude current smokers from this study, as so frequently done.

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:

Q1. no
Q2. no
Q3. no
Q4. no
Q5. no

I declare that I have NO competing interests